ability that there would be just this difference in the amount of rotation of the light in the two cases of the differently disposed molecules, those with their "apices" turned towards the direction of incidence of the light affecting it to a different extent from those whose "bases" were the first to receive it. The theory is well worth following out in the original memoir, many confirmations of it being adduced from other properties of hemihedral crystals.

THE additions to the Zoological Society's Gardens during the past week include a Red-necked Grebe (*Podiceps griseigena*), presented by Mr. Thos. Hardcastle; two Japanese Pheasants (*Phasianus versicolor* ♂ ♀) from Japan, presented by Mr. E. Wormald, F.Z.S.; two Passerine Owls (*Glaucidium passerinum*), European, presented by Mr. St. John Northcote; forty River Lampreys (*Petromyzon fluviatilis*) from British fresh waters, presented by Mr. Thos. F. Burrows; a Black-headed Lemur (*Lemur brunneus* ♂) from Madagascar, a Triton Cockatoo (*Cacatua triton*) from New Guinea, deposited; a Milne-Edwards Porphyrio (*Porphyrio edwardsi*), a Grey-headed Porphyrio (*Porphyrio poliocephala*) from Siam, purchased; a Variegated Sheldrake (*Tadorna variegata*) from New Zealand, an Indian Python (*Python molurus*) from India, received in exchange; an American Bison (*Bison americanus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE TRANSITS OF VENUS IN 1761 AND 1769.—The Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac, vol. ii. Part 5, contain a discussion of observations of the transits of Venus in 1761 and 1769, by Prof. Simon Newcomb. The discussion was undertaken because of the mutations of opinion as to the value of the observations. For more than a century these observations were looked upon as affording the best data for the determination of the solar parallax. Now, however, opinion leans considerably to the opposite direction.

The general theory on which all the previous discussions were founded is said by Prof. Newcomb to be (1) that the formation of the thread of light at ingress and its breaking at egress mark the true time of tangency of the dark limb of Venus with the bright limb of the sun; (2) that a definite interval intervenes between this formation or breaking of the thread of light and the moment known as apparent or geometric contact of the limbs, which interval arises from and depends upon irradiation. A little consideration, however, will show that the time of formation of the thread of light, and therefore the interval between the two phases of contact, must vary with the observer, the quality of the telescope employed, and the conditions of vision. The clearer and steadier the air, and the sharper the vision, the more nearly must the two phases approach each other, until, under the best conditions, they come together. The indefinite and varying character of the phenomena, therefore, favours the division of the observations into the following classes: (1) those in which the formation or extinction of the thread of light is stated to have been observed; (2) cases in which some phenomenon of contact was noted earlier than the formation of the thread of light at ingress or later than its disappearance at egress; (3) observations in which no statement is made from which it can be inferred that one phase rather than the other was observed. These different kinds of observation have been satisfactorily treated by introducing corrections into the equations of condition.

The collected observations take up a large amount of space, and are of prime importance. Their classification and discussion, and the treatment of residuals in the formation of the equations of condition, is also in keeping with Prof. Newcomb's previous work. With respect to the values of the coefficients expressing the effect of imperfections of vision upon the visibility of the contacts, it is concluded that—(1) under the worst ordinary conditions of vision, Venus, at visible external contact, impinged heliocentrically $0''\cdot 7$ further upon the sun than it did under the best conditions, telescopic and atmospheric; (2) in the case of second contact the thread of light, when first seen, was heliocentrically $0''\cdot 16$ thicker under the worst than under the best conditions; (3) in the case of third contact no difference is shown in the thickness of the thread of light under different conditions when it vanished from sight.

When the weights of the observations of contacts III. and IV. in 1761, and contacts I., II., III., IV. in 1769, are considered, the value found for the equatorial horizontal parallax of the sun is

$$\pi = 8''\cdot 79 \pm 0''\cdot 051 \text{ or } \pm 0''\cdot 034,$$

where $0''\cdot 051$ represents the mean error, and $0''\cdot 034$ the probable one.

The mean probable result found by Encke from a discussion of the same observations was $8''\cdot 571$, with an estimated probable error of $0''\cdot 037$, which error was afterwards found to be too small. A variety of causes are put forward to account for the wide difference in the results, and it is hoped that some other astronomer will be able to make a thorough comparison of the two determinations.

Prof. Newcomb also provisionally concludes that the correction to Leverrier's latitude of Venus at the descending node for the mean of 1761 and 1769 is $+1''\cdot 915$, while the correction to Leverrier's longitude of the node (1765·5) appears to be $+32''\cdot 4$.

LEYDEN OBSERVATORY.—The fifth and sixth volumes of the *Annalen der Sternwarte in Leiden* have recently been issued by the Director, Dr. H. G. Van de Sande Bakhuyzen. The observations of stars contained in the former volume have been made with the usual care. The latter deals with the reduction of the zenith distances of several stars.

THE PRINCIPLE OF LAMARCK AND THE INHERITANCE OF SOMATIC MODIFICATIONS.[1]

> "To reject the influence which use or disuse of a part may have on the individual or on its descendants is to look at an object with one eye."—SIR WILLIAM TURNER.

IN last year's lectures we divided the factors of organic evolution into two great classes—firstly, primary factors; and secondly, secondary factors.

The primary factors are those which act directly on the individuals of a given generation; or indirectly on the individuals of the succeeding generation in consequence of a modification of the reproductive elements. Such are light, heat, climate in a general way, nutriment, the nature of the water for aquatic organisms, &c. Further primary factors are the ethological reactions of animals or plants against the inorganic and the living surroundings—what Lamarck called their needs and habits, what Darwin named the struggle for existence, sexual competition, migrations, &c.

We have seen that the action of these primary factors alone and of heredity could give rise to new races and consequently to new species, according to the law of Delbœuf. For this it is enough that the factors act continuously or even periodically, and that the changes produced are not disadvantageous to the modified organism; because, in this last case, natural selection comes into play and rapidly produces an elimination of the less favourable. But the most constant of primary factors of evolution are strongly aided by the secondary ones. These preserve and rapidly augment the results brought about by the primary factors, and determine the adaptation to any given environment of the forms whose variability has been acting. In the case of fully differentiated organisms, *i.e.* all whose parts are specially and rigorously adapted to definite conditions of existence, when a primary factor begins to modify one of these conditions, a return to biological equilibrium is henceforth impossible, and the organism vanishes. This explains the disappearance in olden times of highly differentiated forms (Trilobites, Ammonites, &c.) as a result of apparently unimportant changes in the state of the surroundings. Thus we also can understand how very slight ethological modifications would promptly bring about the annihilation of so highly specialized types as Peripatus, Ornithorhynchus, &c.

But on organisms still possessing a certain plasticity, still having a certain number of elements of unfixed value to dispose of, the action of primary factors causes only a momentary rupture of ethological equilibrium, and consequently more or less extensive variations. Then the secondary factors come in, and eliminate some and fix others of these variations, forming, from a dynamic aspect, new states of equilibrium; and from a morphological aspect, new species. This is the *rôle* of natural selection, of sexual selection, of segregation, and the other secondary factors we shall study this session.

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[1] Prof. Giard's opening lecture, in the Sorbonne, of his course on "Organic Evolution" (chair founded by the City of Paris).

The study of the primary factors of evolution is sometimes called Lamarckism. For Lamarck believed that it was possible to explain the evolution of all organisms by the action of these factors, to which he added heredity alone.

On the other hand, Darwin, while not denying the *rôle* of the primary factors in the production of new species, strove to show that the most important part should be given to natural selection and a small number of other secondary factors. Hence the name of Darwinism given to the study of these factors by many biologists.

Of late, attempts have been made to oppose Darwinism to Lamarckism, or at least to give almost exclusive weight to the one or to the other. Although we have protested several times already against these exaggerations, too often inspired by a deplorable chauvinism, the discussion has become so important of late that it is well to give it a few passing words. Among the disciples of Darwin, some, such as Romanes, have tried to show that natural selection alone cannot explain all the details of organic evolution; and that, along with it, other agents must be considered. Romanes has especially studied a new secondary factor, which he calls *physiological selection*, whose value we shall discuss later in the course. Moreover, Romanes does not reject the influence of primary factors any more than Darwin.

But other naturalists, more Darwinian than Darwin, refuse to admit any other cause of evolution than natural selection. They excommunicate at the same time Romanes, Delbœuf, and the new adherents to the views of Lamarck, revived and placed in touch with modern knowledge. At the head of the ultra-Darwinists is Weismann, who, in his numerous essays, strives to show that the explanations of Lamarck may be replaced by others drawn solely from the mechanism of selection. These essays, partly translated into English, have been received with great favour by the majority of British biologists, and have brought powerful help to the eminent Alfred Russel Wallace, who shares with Darwin in the honour of the discovery of natural selection, and has never ceased to give to this factor an altogether preponderating influence in the formation of species.

In an address delivered at Cologne on September 20, 1888, before the Association of German Naturalists, Weismann went the length of saying:—"I believe I am able to affirm to-day that the material existence of a transmission of acquired characters cannot be proved, and that there exist no direct proofs of the principle of Lamarck."

But what is the principle of Lamarck? That illustrious biologist formulated two fundamental laws. In his own words:—

(1) In every animal which has not got beyond the period of developing, the frequent and sustained use of any organ gradually strengthens it, develops it, enlarges it, and gives it a power proportionate to the duration of the using of it; while the continued disuse of this or that organ imperceptibly weakens it, and it deteriorates, loses its power by degrees, and finally disappears.

This is the law of *ethological reaction* or the law of *adaptation*.

(2) All that Nature has allowed individuals to gain or lose by the influence of the circumstances to which their race has been exposed for a long time, and, consequently, by the effect of predominant use of this organ or continued disuse of that, is conserved in the new individuals who spring from them, and who therefore find themselves better adapted than their ancestors, if the conditions of existence have not changed.

This is the *principle of heredity*, and it is to this law that Weismann alluded when he spoke of the principle of Lamarck.

If this principle of Lamarck is inaccurate and not demonstrable, one sees that the *rôle* of the primary factors is markedly lessened. The transmission of characters specially determined by these factors is no longer a scientific fact; their action is bound to set in motion in a vague way the variability of the germs, without which it is impossible to show a precise nexus of causes, a rigorous connection between the primary factor which acts and the variation it produces.

Before we begin our study of secondary factors we must examine one question—How far must we admit the restrictions to the primary factors brought forward by Weismann, and, before all, what have we to think of the absolute denial of Lamarck's principle?

If we follow Weismann in the very special criticism he has made of this principle, we find, at the outset, that he restricts considerably the limits in which Lamarck applied the law of inheritance of acquired characters:—

"We must not cite," says Weismann, "as facts capable of direct observation, transmissions of acquired characters in cases of injury or of mutilation: the observation of the transmission of functional hypertrophy or of atrophy has never been made, and one scarcely can hope to find it in the future, because it is a territory hardly accessible to observation."

Moreover, Weismann affirms that organs rudimentary through want of usage may be explained perfectly without the intervention of the principle of Lamarck.[1]

He reduces what one commonly calls acquired characters to a very narrow category of modifications, which in no way correspond to what Lamarck intended by the same expression.

In reality, among the many modifications of organisms, often in a vague manner called acquired characters, Weismann distinguishes those which affect the elements of the body—*somatogenic* modifications; and those which influence the reproductive elements—*blastogenic* modifications.

If, for example, a man has a finger amputated, his tetradactylity is a *somatogenic*[2] property; if, on the contrary, a child be born with six fingers, his hexadactylity arises from a special state of the germ, and is a *blastogenic* peculiarity. With this definition and limiting of somatogenic modifications to mutilation and to wounds, with which Weismann is content, then it is certain that the *majority* of somatogenic modifications are not transmitted by heredity.

"It is evident *a priori*," as Duval justly remarks ("Le Darwinisme," p. 309), "that only those variations can be inherited which have their source in an influence affecting the whole organism in such a fashion as to bring about profound transformations, of which the variation in question is a local manifestation. And indeed, if that modification be simply a local manifestation of a general tendency of the organism, it is, however, true that descent may transmit merely the tendency, which only shows itself later in the subsequent products of the variation in question. It is this that is presented to us in the case of atavism, in which the variation jumps over one generation.

"But a sudden accident, such as a blow which destroys part of the organism, does not result in a modification of the whole organism, and hence does not represent any general tendency nor any chance of forming an inheritable mutilation. The gardener, in slowly modifying plant or shrub by special conditions of culture, brings out variations which he may hope to see reproduced in the descendants; but when he capriciously prunes the branches of a shrub, he knows well that neither by slippings nor by seedlings can he get produced from that shrub, deformed by the sharp instrument, new plants which will bud forth similar deformations."

Thus Weismann seems to have given himself much trouble with meagre result in his discourse "On the Hypothesis of a Hereditary Transmission of Mutilations." In such a subject each case must be studied separately, and if, after cutting the caudal appendices from five successive generations of white mice, Weismann observed no modification with the descendants of these animals, that only proves that the sectioning of a mouse's tail carries with it no profound modification of the organism of these animals.

Similarly, all the discussion about the tailless cats of the Isle of Man and of Japan seems to us very ably and logically conducted, but the conclusions drawn therefrom do not go beyond the compass of that particular case. Among the feline species, at least in a domestic state, the existence of a caudal appendage more or less developed is of very secondary significance, and the artificial selection by man, guided by caprice or prejudice, may lead to the disappearance of that appendage in certain localities, particularly in islands.

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[1] The demonstration of this assertion which Weismann has tried to give does not seem to me sufficient. I do not believe it is any more justifiable to make the assertion that the principle of Lamarck will be inapplicable to many instincts, in particular to those which appear only once in the life of an animal. But this discussion will be more *à propos* when we are studying the laws of heredity.

[2] We keep Weismann's terminology, though "somatic" seems a better word.

There is a whole series of facts which Weismann might have cited in support of his manner of view, but which do not furnish any argument more demonstrative against the inheritance of somatogenic modifications, if one gives to the word a meaning wider than that of simple mutilation. I speak of phenomena so curious as voluntary mutilation or autotomy, of which I recently pointed out the frequency and variety.[1] Innumerable generations of lizards have voluntarily broken off their tails to escape from various enemies, without that appendage ceasing to reappear among their descendants. At the most, one may say that selection has rendered this mutilation more easy and more frequent with certain species of saurians, as with certain echinoderms, certain molluscs, &c. The organism has acquired the power of parting easily with this or that part, while the part, sometimes seemingly without any use, does not fail to reappear in each new generation, because its suppression produces no reaction in the other organs.

But this is not always the case. Mutilations and wounds, which at first seem of little importance, nevertheless call forth somatogenic modifications as often inherited, because they give to the organism affected a disturbing action, which probably extends to the reproductive elements.

Weismann has not even made allusion to cases of this sort, of which a certain number were noted by Prof. Brown-Séquard long ago.[2]

Here are the leading varieties of the inheritance of the effects of accidental injuries, as given by that investigator :—

(1) Epilepsy in the descendants of guinea-pigs, male or female, in whom it was originally produced by cutting the sciatic nerve or the spinal cord.

(2) A singular change in the shape of the ear, and a partial shutting of the eyelids in the descendants of individuals (guinea-pigs) which had these as the result of cutting the cervical sympathetic nerve.

(3) Exophthalmia in the descendants of guinea-pigs which had this protrusion of the eye after an injury to the fourth ventricle.

(4) In the descendants of certain individuals in which a lesion of the restiform body had been produced, there occurred ecchymosis, followed by dry gangrene, besides other changes of the blood-supply to the ears.

(5) Absence of phalanges or whole toes of the hind paws in descendants of guinea-pigs which had accidentally lost their toes after cutting the sciatic nerve.

(6) Morbid state of the sciatic nerve in the descendants of individuals which had had that nerve divided, and the successive appearance of the phenomena, described by Brown-Séquard as characteristic of the periods of development and of abatement of epilepsy, and especially the appearance of an epileptic area in a part of the skin of the head and neck, and of the disappearance of hair around that area the moment the disease showed itself.

(7) Muscular atrophy of the thigh and leg of guinea-pigs, offspring of individuals with muscular atrophy following resection of the sciatic nerve.

(8) Defect in one or even both eyes of guinea-pigs whose parents had an eye deteriorate after the cutting of the restiform body.

Prof. Brown-Séquard has shown that the inheritance of the morbid conditions mentioned above may manifest itself in one side only, while both sides were affected in the parent. The inverse may also exist. Further, if the parent and descendant both have the morbid state in only one side, it sometimes happens that the side is not the same in both cases. The inheritance of these conditions may be wanting in one generation, and appear in the succeeding one. The female is better able to transmit morbid states than the male. As to the frequency of these transmissions, Prof. Brown-Séquard affirms that with more than two-thirds of the animals born of parents which have had several of these morbid conditions resulting from accidents, such modifications have shown themselves. The transmission by heredity of several of these pathological states may happen for generations, and has been proved to the fifth and even to the sixth generation.

These interesting facts have been confirmed since by Mr. E. Dupuy, who, further, has tried to explain them by an alteration of nutrition. One is astonished that none of the naturalists who have taken part in the discussion about the transmission of acquired characters so long carried on in NATURE, has thought to verify or even to discuss these facts.

After the preceding it seems to me that the partisans of Weismann's ideas have not paid sufficient attention to the marked reactions which certain somatogenic lesions may have on the modified organism, and in turn on the descendants.

Recently the botanists have given still more curious examples of the transmission of acquired characters. Certain somatogenic modifications produced by the slow action of parasites or symbions on various plants are capable of being transmitted by heredity. Scarcely four years ago Duval could write : "The oak and other trees have certainly borne galls since most primæval times, and yet nobody expects to see them produce inherited excrescences without the intervention of insects whose puncture is the origin of the galls." To-day that cannot be said of all galls and gall-like productions. Since then I have shown that, in a large number of cases, these productions profoundly modify the organism affected, and give rise to phenomena so singular that I have described them under the name of *parasitic castration* (*castration parasitaire*).

According to the excellent researches of A. N. Lundstroem, the deformations (*trichomes*), produced on the leaves of the lime and several other trees and shrubs by the prick of the arachnidans, are perfectly inheritable, even when those plants are grown protected from the parasites which have caused the modifications in the ancestors.

According to the researches of Treub and other botanists, the same is true for the singular transformations (*myrmecocecidies*), resulting from the action of the ants on several tropical plants.

Even whilst holding to the action of the most common primary factors, we believe we can establish in a stable fashion the transmission by heredity of somatic modifications. A certain number of acquired characters, which are specially revealed by somatogenic peculiarities, are moreover accompanied by correlative blastogenic modifications (and not merely consecutive as in the preceding cases) of such a nature that it becomes impossible to make the distinction proposed by Weismann, and these characters are justly considered inheritable by a majority of naturalists.

As in these examples the primary factors have modified at the same time the individual and the future product, the application of the principle of Lamarck cannot be disputed in the least degree. We invoke here the testimony of a naturalist, little suspected of partiality for the idea of evolution in general, and for Lamarck in particular. Godron, in his book "Sur l'Espèce et les Races chez les êtres organisés" (vol. ii. pp. 7, 8), tells how, according to Bishop Heber, dogs and horses taken from India into the Cashmere mountains become covered with wool ; and how domestic animals in the tropics have their hair shortened and stiffened (sheep in Peru, Guinea, &c., merinos in South Sea Islands), or disappearing altogether (dog of Guinea, cattle of South America). The inverse action is not shown in our domestic animals brought from the tropics, which, says Godron, "proves that the action of climate is not always immediate or absolute."

Do not these last cases show that the modification produced is not due simply to the action of primary factors on the individuals, but that the blastogenic properties have been equally affected, and it follows that the principle of Lamarck finds its application ?

Finally, if for certain plants modified, be it by their habitat among the mountains, or by their habitat on the sea-shore, the return to the normal type may take place the first generation ; one knows that there are others with whom this return cannot be effected until after a long series of cultures. What cultivator does not know that there is a chance to keep up any race by taking for progenitors only the individuals showing most markedly the characters of that race ? While in most cases the domestic races have been produced simply in view of the modifying of certain somatogenic characters, the breeder at the same time has produced unconsciously the correlative blastogenic modifications which insure the transmission of these somatogenic peculiarities.

But while there is acting the primary factor of the ethological reaction, that which Lamarck specially had in view, we may prove also the transmission of acquired character, or, if one prefers to employ Weismann's terminology, the concomitance in the parent of somatogenic modifications and of blastogenic modifications, destined to cause the reappearance in the offspring of somatogenic modifications of the same nature, even when the causative factor has ceased to act on the offspring.

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[1] Giard, "L'Autotomie dans la série animale" (*Revue Scientifique*, 1887, vol. xxxix. p. 629).

[2] See especially Brown-Séquard, "Faits nouveaux établissant l'extrême fréquence de la transmission par hérédité d'états organiques morbides, produits accidentellement chez des ascendants" (*Comptes rendus de l'Académie des Sciences*, March 13, 1882).

Godron (*loc. cit.*, ii. 24), shows how the close relationship existing between muscles and skeleton makes any great change in the former affect the latter, and therefore the external appearance of the animal. The confinement of Brahmaputra and Cochin-China fowls prevents the use of the pectoral muscles, which become smaller, the wings also shorten, and the birds lose the power of flight, while the law of balancing of organs demands a development of the lower members. He also notes the change of shape of the horse according as it is used for riding or haulage; and the effect of continuous milking on secretions and development of mammifery organs of cows, and the diminishing of the udder and stoppage of milk-giving as the calf takes to cropping the grass, when continuous milking has ceased for several generations (*e.g.* on a return to wild conditions).

Among contemporary physiologists, Prof. Marey has insisted on the casual connection which exists between the animal mechanics and comparative morphology. While recognizing the importance of the facts already known, he has not ceased to demand new experiments for the purpose of knowing if the modifications one can produce on an animal by an exaggerated exercise of certain muscles cannot possibly be transmitted to its descendants. "One cannot yet affirm," he says, "but it is very probable, that the evolution theory will receive this last confirmation."

In this matter, as in many others, if evolutionists must content themselves in most cases with experiments unconsciously carried on in Nature, or those of breeders, instead of applying themselves to verifications made with all the rigour of modern scientific precision, is it not because of the deplorable insufficiency of our laboratories? One is astonished that in no country, not even where science is held in greatest honour, does there yet exist an *institut transformiste* consecrated to the long and costly experiments now indispensable for the progress of evolutionary biology.

The partisans of Weismann's ideas raise the objection that, in all the cases mentioned above, it is not the somatogenic character, but a blastogenic property, in virtue of which the descendant is susceptible of being impressed by the primary characters which determine that somatogenic character to the same and even to a superior degree to its parents.

This likeness, or rather harmony, between blastogenic modification and correlative somatogenic modification, is quite inexplicable if one will see nothing but a simple coincidence, accidental in origin, and fixed only later by selection. In reality, all takes place as if the somatogenic character were itself inherited, and putting aside all theoretical bias, it appears much simpler and more exact to explain the matter in this fashion. What else is it but heredity, this reappearance in the offspring at a given moment of physico-chemical or mechanical conditions, identical with those which have determined in the parent a morphological and physiological condition, resembling that which showed itself at a like moment in the progeny. Unless you attribute to the phrase *blastogenic modification* a mysterious and extra-scientific meaning, to speak of inherited blastogenic properties is simply to say that the order of the mechanical states which will be realized later in the development of a living being is already contained in the germ in a potential condition. Consequently, to say that at a given moment an animal inherits the possibility of losing its hair under the influence of heat, is equivalent to saying that it inherited the loss of hair which happened to its ancestors under the same conditions. When one goes to the root of the matter, the discussion becomes a simple dispute about words.

Besides, as Sir William Turner noted in a remarkable lecture on heredity, there are other facts which show that the separation of the reproductive and the somatic cells is not so absolute as Weismann and his followers seem to admit. If, in some animals, *Moina* for instance, the separation of the genital cells happens so precociously that they can already be distinguished in the first stages of the segmentation, one can affirm that in the majority of cases these cells come from somatic cells, and their plasma has passed through innumerable cellular generations before becoming the specialized sexual individuals of the colony.

In certain organisms, and particularly in plants, it even seems as if any somatic cell whatever is capable, in certain definite cases, of behaving like a parthenogenetic genital cell, and of completely reproducing the organism. Sachs has shown this for certain cells of the roots, of the leaves, and of the buds of several mosses.

One also knows when leaves of *Begonia* are cut, and the pieces grown in a hot-bed, one can get new stems which will bear flowers and fruits.

Undoubtedly it would be the same with some animals whose regenerating powers are highly developed (*e.g.* Turbellarians and certain Oligochætes) if one could nourish sufficiently the artificially separated bits. Theoretically one may say that each cell of a Planarian possesses in itself all that is needed for the reproduction of a new individual.

How is one to admit that a modification of somatic cells will not be followed by a correlative transformation of the product and of its blastogenic cells?

Variations produced by budding give us similar arguments, and show clearly the influence which the modification of certain somatic cells may have on other somatic cells and on the reproductive cells.

More interesting still, from the same point of view, are certain observations on the influence which the grafted subject may have, not only on the somatic elements, but even on the fruits of the graft. Darwin says:—

"Several North American varieties of the plum and peach are well known to reproduce themselves truly by seed; but Downing asserts, 'that when a graft is taken from one of these trees and placed upon another stock, this grafted tree is found to lose its singular property of producing the same variety by seed, and becomes like all other worked trees;'—that is, its seedlings become highly variable. Another case is worth giving: the Lalande variety of the walnut-tree leafs between April 20 and May 15, and its seedlings invariably inherit the same habit; while several other varieties of the walnut leaf in June. Now, if seedlings are raised from the May-leafing Lalande variety, grafted on another May-leaving variety, though both stock and graft have the same early habit of leafing, yet the seedlings leaf at various times, even as late as June 5" ("Animals and Plants under Domestication," 2nd ed., 1875, vol. ii. p. 247).

Inversely, the graft may communicate to the subject grafted certain somatic modifications with which it itself is affected. For instance, one knows that when one grafts the variegated variety of jasmine on the ordinary form, the latter sometimes bears buds with variegated leaves. The same is true of the laurel and of the ash. One has been able to produce even a half-breed of the graft, and perhaps the most curious are those obtained by Prof. Hildebrand at the request of Mr. Darwin. After having raised all the eyes of a smooth-skinned potato, and also those of a rough-skinned red potato, Hildebrand inserted them the one in the other reciprocally, and succeeded in raising two plants. Among the tubers produced by those two plants he found two red and scaly at one of their extremities, one white and smooth at the other, the intermediate portion being white and marked with red stripes.

These last examples bring us to cite facts of another nature, still insufficiently known to-day, but which seem to prove in an unexceptionable manner the influence of the somatic on the blastogenic cells. I am speaking of what Darwin called the direct action of the male element on the maternal form and even on the ulterior products.

"Even as long ago as 1729 it was observed that the white and blue varieties of the pea, when planted near each other, mutually crossed, and in the autumn blue and white peas were formed within the same pods." But this modification of the colour of the fruit may extend even to the husk, *i.e.* to the somatic cells of the maternal organism. "Mr. Laxton has cultivated the tall sugar-pea, which bears very thin green pods, becoming brownish-white when dry, with pollen of the purple-podded pea, which, as its name expresses, has dark-purple pods with very thick skin, becoming pale reddish-purple when dry. Mr. Laxton has cultivated the tall sugar-pea during twenty years, and has never seen or heard of its producing a purple pod; nevertheless, a flower fertilized by pollen from the purple pod yielded a pod clouded with purplish-red" (Darwin, *loc. cit.*, i. p. 428).

Numerous analogous examples of the action of the pollen of certain plants on the ovary of neighbouring varieties, have been collected by Gallesio, Naudin, Anderson, &c. Only recall the famous apple-tree of Saint-Valery, so carefully studied by Tillett of Clermont-Tonnerre. This tree did not produce pollen in consequence of the abortion of its stamens, and had to be artificially fertilized each year. The operation was done by the young girls of the district, by means of pollen taken from different varieties. Fruits of various varieties and sizes, colour and flavour, resulted, resembling the sorts which had furnished the pollen.

As the ovary of plants perishes after the production of the fruit, and presents transitory connections with the plant itself, it is not probable that the somatic modifications produced by the pollen extend to the cells of the branch or of the stem. For the same reason these modifications cannot have a reaction on the subsequent products.

But with animals, and especially with the Mammalia, where the fœtus is for a long time in intimate connection with the mother, one may suppose that the action of the male element will have an influence on the maternal organism first, and later on the subsequent descendant.

"In the case often quoted from Lord Morton, a nearly purely-bred Arabian chestnut mare bore a hybrid to a quagga; she was subsequently sent to Sir Gore Ouseley, and produced two colts by a black Arabian horse. These colts were partially dun-coloured, and were striped on the legs more plainly than the real hybrid, or even than the quagga. One of the two colts had its neck and some other parts of its body plainly marked with stripes. Stripes on the body, not to mention those on the legs, are extremely rare . . . with horses of all kinds in Europe, and are almost unknown in the case of Arabians. But what makes the case still more striking is that in these colts the hair of the mane resembled that of the quagga, being short, stiff, and upright. Hence there can be no doubt that the quagga affected the character of the offspring subsequently begot by the black Arabian horse" (Darwin, *loc. cit.*, i. p. 435).

Turner, who recalls Darwin's example, found the assumption that attributes the presence of the stripes to a reversion towards an ancestor common to horse and quagga too complex and hypothetical. He believes that the mother, while she had the hybrid in her womb, acquired from it the faculty to transmit the characters of the quagga, owing to the necessary nutritive changes during the development of the fœtus. The germinative plasma of the mother, belonging to the ovules not yet ripe, will have been modified in the ovary itself; and this acquired variation will have its reaction on the later-born individuals of the same mother.

The same explanation has been admitted by other physiologists for similar facts often proved by breeders and by hunters, for different domestic animals, and especially for dogs. Indeed, one knows that when a bitch has been covered for the first time by a dog of another breed, its subsequent offspring will show one or more little peculiarities belonging to that other breed, although she has been covered afterwards by dogs of her own race.

The accuracy of this hypothesis will be strongly restricted if, as certain observers, such as Mr. Chapuis, affirm, the influence of the first male also shows itself in the case of birds (pigeons) where the relations between the mother and the little ones are much less intimate than among the Mammalia. But, however it be with this explanation, the fact itself, outside of all theory, sufficiently shows the close connection which exists among the reproductive and the somatic elements.

In order not to leave the domain of facts scientifically established, or hypotheses susceptible of a more or less easy proof, I shall set aside the influences which impressions produced on the senses and nervous system of the mother may have on the offspring. The popular belief in these influences is very ancient, for we read in Genesis that Jacob placed rods whose bark was marked in various ways before his father-in-law's sheep, in order to get certain markings on the lambs which the ewes bore. But the antiquity of a belief is not always a proof of its accuracy, and I admit with Weismann, that the cases quoted to prove the transmission of physical characters are not convincing, even in a case as curious as that of the mare of Baer.

Nevertheless, it seems to me very difficult to admit that the emotions and psychical impressions, which act so energetically and so evidently on all our secretions, have no influence on the products of the genital glands. Perhaps, outside the conditions of the surroundings and of education, which should be put in the first rank, one may attribute to an action of this sort the fact that all of one generation accept with the greatest ease the ideas which had been warmly combated and repelled by the preceding generation. It seems to me impossible that the intellectual movement caused by men of genius in one or more branches of human knowledge—a movement propagated and disseminated by men of letters and artists—has no reaction on the blastogenic elements of the contemporary generation, and consequently on the generation following, which thus will be prepared by hereditary transmission for an altogether new order of psychic modalities.

Finally, a last consideration leads us again to combat the opinion of those who maintain that acquired somatogenic characters cannot be transmitted from parents to infants. If, as already quoted from Turner, we push this theory to its final consequences, we are led to suppose that the ancestors of actual living beings, and the primordial protoplasm itself, possessed in themselves all the variations which have appeared since; and as, according to this hypothesis, the primary factors have acted only on the individual and not on the blastogenic elements (which alone can be transmitted), we must conclude that these possessed from the beginning, *i.e.* from the appearance of living matter, an evolutionary potency in a certain measure indefinite. We shall thus be led to the idea of creative forces, regulated, it is true, by selection. Thus again the door will be open to directing agents, immanent or outside of matter, and we shall require to renounce the grand mechanical conception of the universe, seen by Descartes, and followed up by the inquirers of the eighteenth century.

If, on the other hand, we admit the transmission of somatogenic characters in the measure proved by the facts cited above, the transformation of living beings will become much quicker, because it will not depend entirely on the chances of internal variation, but will be determined by the action of primary factors.

The *rôle* of selection and of secondary factors will remain most important, accelerating and regulating the transformation.

But before passing to the examination of secondary factors, we shall first study a biological phenomenon which we find whenever new organisms are produced—hereditary transmission. In explaining the production of these forms, whether we make use of the principle of Lamarck, the law of Delbœuf, or the selection of secondary factors, we have seen we always must admit the action of heredity.

Properly speaking, heredity is neither a primary nor a secondary factor. It is the integral, the sum of indefinitely small variations, produced on each anterior generation by the primary factors. The laws of heredity, hardly studied yet experimentally, offer a vast field for the sagacity of biologists. Several of these laws, and especially the law of homochronic heredity (*l'hérédité homochrone*), give good arguments for the principle of Lamarck. The most recent embryological researches begin to afford us a little insight into the mechanical process of hereditary transmission, and the deeper phenomena of reproduction.

It is only after carefully examining all the information acquired on these delicate matters that we can begin to study the secondary factors of evolution with profit.

THE INSTITUTION OF MECHANICAL ENGINEERS.

ON Thursday and Friday of last week, the 29th and 30th ult., the forty-fourth annual general meeting of the Institution of Mechanical Engineers was held in the theatre of the Institution of Civil Engineers, the latter society extending their usual hospitality to the sister Institution.

There were but two papers on the agenda, viz.:—On some different forms of gas furnaces, by Mr. Bernard Dawson; and on the mechanical treatment of moulding sand, by Mr. Walter Bagshaw, of Batley.

The fourth report of the Research Committee on Friction, which deals with experiments on the friction of a pivot bearing, was also down for reading, but time did not permit of it being taken. This was chiefly due to the fact that Mr. Macfarlane Gray proposed an alteration in the bye-laws, in virtue of which any full member who had paid thirty annual subscriptions—£3 each—should become a life member without further payment. This proposal received influential support in the course of a long discussion, but was nevertheless negatived on a division by a considerable majority.

The President, Mr. Joseph Tomlinson, occupied the chair on both evenings.

Mr. Dawson's contribution was one of considerable interest,